Discussion before the Wireless Section, 1st April, 1931.

Dr. R. L. Smith-Rose: With regard to the second paper, it is interesting to compare the results obtained by the author in Cornwall when making field-strength measurements on Daventry 5XX with those given in the recent paper* by J. H. Reyner. A comparison between Fig. 3 of Reyner's paper and Fig. 2 of the author's leads one to the conclusion that either the radiation from Daventry has altered considerably since the earlier measurements were made, or—what is more likely—one of the two investigators has made serious errors in his measurements. On page 128 the author hints that the accuracy of his measurements is of the order of 5 per cent, and as Reyner at the end of his paper makes a 4 per cent correction to the experimental values, I presume he also regards his results as correct to, say, 5 per cent. Reyner's values for the field strength in the neighbourhood of Bodmin are more than twice the author's figures, and the difference in shape of the contours shown in the two papers is considerable. The author draws the conclusion that the effects which he observed in Cornwall are due to the hill known as "Brown Willy," which is 1 400 ft. ($0{\cdot}25\lambda$) high. I find it difficult to conceive that a hill of this height could cast over the rest of Cornwall a shadow persisting for some 50 miles. I should have thought that the dimensions of this hill would have been insignificant compared with the distances over which the measurements were taken in the south-west of Cornwall. Reyner, on the other hand, attributed his peculiar results to the effect of the Bodmin beam station. Although the height of a beam-station aerial is small (about $0{\cdot}1\lambda$), the span of a beam system is considerable. The total span of the two banks of the Bodmin aerial system is about 6 000 ft. (just over a wavelength), and while I should not like to rule out the possibility that this aerial may be producing effects upon the field intensity in other parts of Cornwall, I feel doubtful about either explanation. One might suppose that the subsoil of that part of Dartmoor over which the transmission has to pass before it reaches Bodmin would produce some effect, but on the other hand we have hitherto always considered that so long as the subsoil was covered by a depth of soil sufficient for vegetation to exist, the relatively high conductivity of this surface soil would screen the subsoil and prevent it from having any action upon the waves as they passed over the surface. It is possible that the broken nature of the ground in that part of Devon and Cornwall may be the cause of all the effects observed. On page 130 the author refers to measurements made at a number of places which are not indicated on the map, and it would perhaps be advisable to indicate their positions for the benefit of those who are not familiar with the district in question.

I disagree with the opening sentence of the first paper, which states that there has hitherto been no evidence of the influence of downcoming waves from the upper atmosphere in the daytime on long wavelengths at distances up to 300 km. In the first place, Hollingworth's measurements which were made in this country (in daylight) and published in 1926* show a curve with a dip and a maximum in it, instead of a straightforward attenuation curve. The only explanation which was forthcoming for this curve was that it was due to interference between the ground wave and a downcoming wave reflected from the Heaviside layer. Additional evidence of the same phenomenon is supplied by some of my own work on direction-finding.† Fig. A shows daily bearings on the transmissions from Leafield (λ = 12 400 m) taken at Slough at 2 o'clock every afternoon over a period of 15 months. During the winter months appreciable variations in the bearings due to the effect of the reflected waves from the Heaviside layer in the daytime were obtained. The dotted horizontal line through the middle of the results represents the true bearing, and the observed bearings are almost equally displaced on either side of this, except during the summer when there appears to be a deviation of the bearing in a clockwise direction. Thus my experimental results in the summer time may be taken as a partial confirmation of the author's observation that on Rugby transmissions (λ = 18 000 m) the bearing was always rotated in a clockwise direction. This conclusion might, however, be vitiated by the possibility of a permanent error in bearing of a few degrees due to local conditions.

* *Journal I.E.E.*, 1930, vol. 68, p. 181; also *Proceedings of the Wireless Section*, 1930, vol. 5, p. 37.

* *Journal I.E.E.*, 1926, vol. 64, p. 584; also *Proceedings of the Wireless Section*, 926, vol. 1, p. 57.

† Department of Scientific and Industrial Research (Radio Research), Special Report No. 5, 1927.

The author's paper demonstrates that, even though a transmitting station may be radiating equally in all directions, the transmission of energy in the north-south direction is greater than that in the east-west direction at short distances. I think it is dangerous to conclude, however, that because more energy is returned in the north-south direction at short distances, less is available to be transmitted at longer distances. It does not follow that because the energy has not come down at a short distance it will come down a long way away; it may be entirely absorbed. With regard to Figs. 3 and 4, I question the utility of inserting the Austin-Cohen values. I should like to express my high opinion of the results obtained, because I am familiar with the great difficulties

As to the question of whose measurements are right, I have no hesitation in accepting the author's, because his facilities are undoubtedly better than mine were. There is one question I should like to raise, however. If the unusual effect observed is caused by a radio shadow of some natural phenomenon like " Brown Willy," ought not the signal-strength contours to close again rather in the manner which I, in my ignorance, showed them to do? My original paper suggested that if there were a shadow it should close up, and a band of high pressure should be formed again before the signal strength fell off. I looked for this and found what I thought was evidence of its existence, but my measurements were taken in a short time and did not, I think, number more

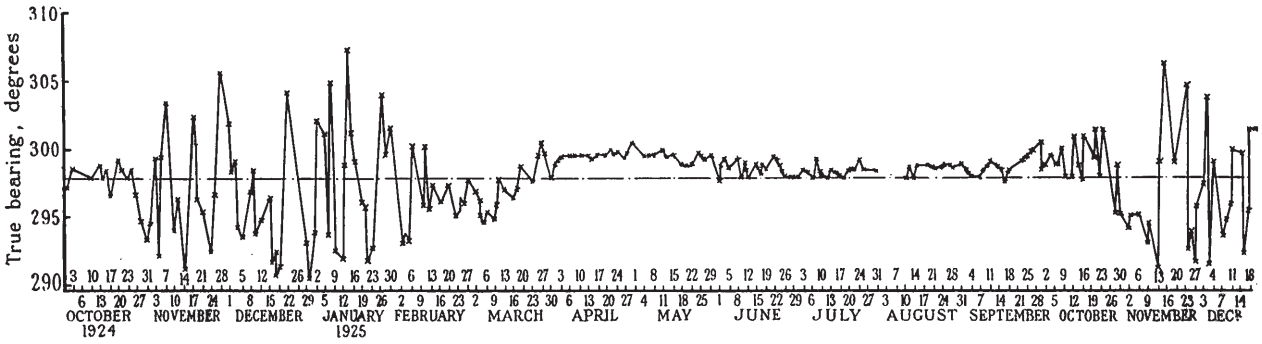

FIG. A.—Graph showing daily observations of bearings taken at Slough on the U.R.S.I. transmissions from Leafield, October 1924 to December 1925.

$\lambda = 12 \cdot 4$ km, transmissions at 14 00 G.M.T.

there are in making consistent measurements, not only at a fixed station but also when touring the country with apparatus which must be transportable.

Mr. J. H. Reyner: With regard to Dr. Smith-Rose's remark concerning the 4 per cent accuracy mentioned in my paper,* this figure was an estimated attenuation obtained from a theoretical formula, which fact I allowed for when I was determining the absolute signal strength of Daventry. My measurements gave a signal strength of 13 millivolts per metre, which is in satisfactory agreement with the author's figures. While I am a little disappointed at the somewhat serious discrepancy between the author's and my own results in Cornwall, it should be remembered that my measurements were made entirely on my own initiative as a summer-holiday task, that I was only able to spend about three weeks on the work, and that I used very simple apparatus. In the circumstances I am relieved to find that the author did definitely notice a drop in signal strength in the interior of the county as he crossed from coast to coast. My apparatus consisted of a small portable aerial set up on each site. The signal strength from Daventry was allowed to produce a given deflection in the detector circuit; I then changed over to a local oscillator and adjusted this to give the same deflection. To do this I had to use a dummy aerial, because I could not induce the signal in the main aerial while I was still receiving a signal from Daventry. So far as I can see, this is the point where the discrepancy was introduced—although I endeavoured to anticipate and avoid any such error.

* *See* footnote on page 131, col. 1.

than 40, whereas the author has taken very many more. Purely from a theoretical point of view, I should like to know whether this closing of the contour and subsequent slight rise in signal strength ought not to be observed.

Dr. J. Hollingworth: With regard to the first paper, if we consider long waves to be merely channels of communication we probably know everything about them that it is necessary to know. Regarding them as electrical phenomena, however, there are a great number of details which are still crying out for investigation. It may seem rather peculiar that there should be any reason for a sharp distinction between the kinds of phenomena observed above and below 300 km. This distinction, however, is a very important one, and it is emphasized by the following short report dealing with some of the subsidiary phenomena mentioned in the paper by myself to which Dr. Smith-Rose has referred. In connection with a journey north on which I was measuring the signal strength from Rugby and various other stations, the report states: " The results on Rugby yielded a new factor of importance. Up to a distance of 340 km from the transmitter the results, though somewhat irregular, showed nothing very unusual; but at the next reading, only 55 km further north, the intensity dropped from 8 000 to 2 800 microvolts per metre, round about which value it remained for the rest of the journey, and fitted in very closely with the routine figures being obtained at Aberdeen. In order to see if this was merely a temporary phenomenon, I returned about a fortnight later by an entirely different route. The same type of effect was observed, signals rising from an intensity of

5 300 microvolts per metre at 345 km to 10 000 at 305 km." This is very strong evidence that, due to some cause of which we do not know, at 300 km there is a complete change in the phenomenon, and there is reason to believe that it is fundamentally a change of quality rather than of quantity. It is difficult at present to see how to explain this change, but we should guard against the danger of trying to force the facts to suit existing theories. It is more important to recognize that this definite experimental effect occurs; we shall probably have to turn to the mathematical theory for an explanation, and this presents a difficult problem. At one time I was working on long waves, because I thought they were more particularly adaptable to mathematical estimate, but I am now told that the mathematical theory of short waves is the simpler. It is also rather striking that whereas these short- and long-distance phenomena seem to be somewhat alike, the middle-distance ones are entirely different.

Turning to the second paper, the field work involved in these investigations must have presented great difficulties. It was at one time considered that in long-wave measurements at any rate one could obtain reliable readings provided that there were no rivers, railways, or telegraph and telephone lines in the immediate neighbourhood, but these results show that there is some other cause of inaccuracy either above or below the ground.

Captain P. P. Eckersley: Measurements of effective height of aerials taken by the B.B.C. showed certain curious ambiguities. At a point about 10 km from Daventry, measurements gave an effective height of 100 m, indicating a radiation resistance greater than the resistance of the aerial. At 30 km from Daventry the effective height had apparently been reduced to about 80 m. It is interesting to note on page 128 of the second paper that the effective height (not actually mentioned) was half the actual height. The figure obtained would, I suppose, be about 60 m, which is very different from our readings near Daventry. We have frequently observed that the effective height measured close to an aerial appears greater than it should be according to theory. Incidentally, the effective-height reading mentioned in the paper will have to be multiplied by 0·9 (assuming the conductivity to be 10^{-13} C.G.S. units), on account of the slight attenuation introduced between the points of initial radiation and field measurement. I should like to know on what theory the curve shown in Fig. 3 of the second paper is based. Is it the Sommerfeld, Austin-Cohen, or the curvature theory? It is interesting to find that, on the average, the author's results in Cornwall closely approximate to figures obtained by methods outlined in my paper on "The Design of Transmitting Aerials for Broadcasting Stations."* The Sommerfeld theory, as first adapted by Mr. Barfield, is basic to the methods outlined in my paper. In making a study of the effect of hills upon field contours I found that the field on the height of land was always about 60 to 70 per cent greater than that measured on any open space in the neighbourhood. Where the land rose precipitously, the field readings showed a 200 per cent increase above those taken on the plain below. This effect was particularly noticed at Sutton Bank, Yorkshire, a large plateau ending in a precipice overlooking the plain which stretches away in the direction of Daventry. I also found that the field in a valley running in the direction in which the waves were travelling was much less than that in a valley running at right angles to the direction of wave travel. I furthermore noticed that hills caused considerable shadows, but these shadows were local and disappeared some distance behind the obstacle. As a result of a tour throughout the length of the Pennine Range I came to the conclusion that there is an increase of field on the height of land, but that "broken" country is responsible for a greater attenuation than flat ground of the agricultural type. It is noteworthy that whereas the detailed contours extrapolated by two independent observers (J. H. Reyner and the author) are noticeably different, a "general" contour arrived at on the basis of my methods for calculating the service area of broadcasting stations is confirmed by the readings taken by both investigators. It is extremely difficult to express the intensity of the resultant field except in terms of generalities; nevertheless all the work which has been done on the subject since the publication of Mr. Barfield's original contribution is useful since it expands our knowledge and helps us to understand the reasons for the variations of field from a general mean value.

Mr. H. L. Kirke: In connection with the second paper, the B.B.C. recently carried out some work over similar country in Somerset on shorter waves (300 m), for which the effect is considerably more pronounced. When prospecting for a site for the Western Regional station we found that on the top of the Brendon Hills the attenuation for the first 7 miles was far greater than we had ever experienced before. On a wavelength of 300 m over ordinary country one expects to obtain an inverse-distance law, for the first few kilometres at any rate. Possibly at 7 miles an additional attenuation of 10 per cent is to be expected, but in this particular case the wave was attenuated to one-seventh of the predicted value. On referring to a geological map we found that the point where the rapidly attenuating curve gave place to a curve of reasonable form was where the subsoil changed from Devon red sandstone to a soil which was apparently of greater conductivity. As we advanced in the direction of Plymouth we encountered the Dartmoor forest with its subsoil of granite and the signals disappeared completely. It seems to me, therefore, that the effect noticed by the author in Cornwall may have been due to the subsoil, and the reason why the intensity was greater nearer the coast is that the signals come in from the sea. "Brown Willy" probably produced only a local effect. Fig. B shows the attenuation plotted in terms of field strength × distance, so that, apart from additional attenuation due to losses in the ground, the curve should be a straight line. The actual curve changes its general direction at about 8 km, where it becomes fairly straight. At this point (see Fig. C) the sandstone ends, and the signal strength disappears completely where the granite starts. Fig. C is taken from the Geological Survey map. The shadow effect of hills seems to be very similar to that caused by a heavily attenuating piece of ground, or a town. We

* *Journal I.E.E.*, 1929, vol. 67, p. 507; also *Proceedings of the Wireless Section*, 1929, vol. 4, p. 51.

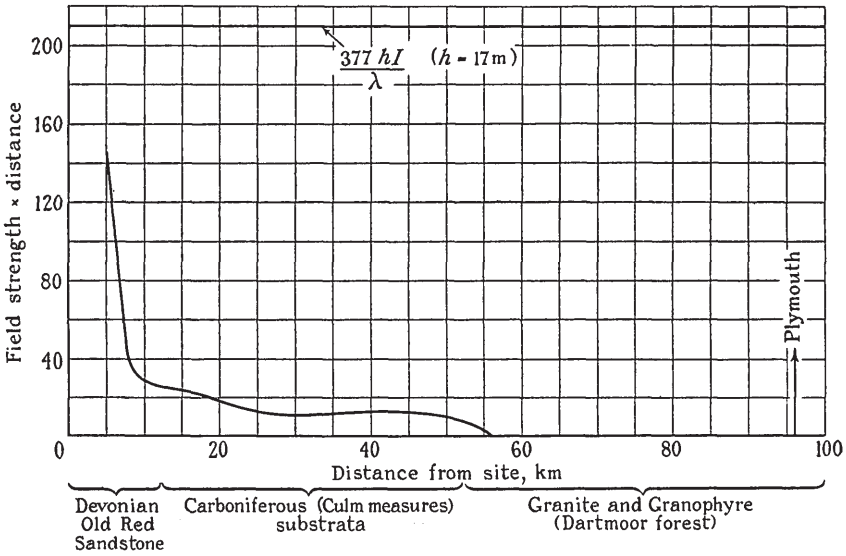

FIG. B.—Attenuation curve, Brendon Hills—Plymouth. $\lambda = 261$ m.

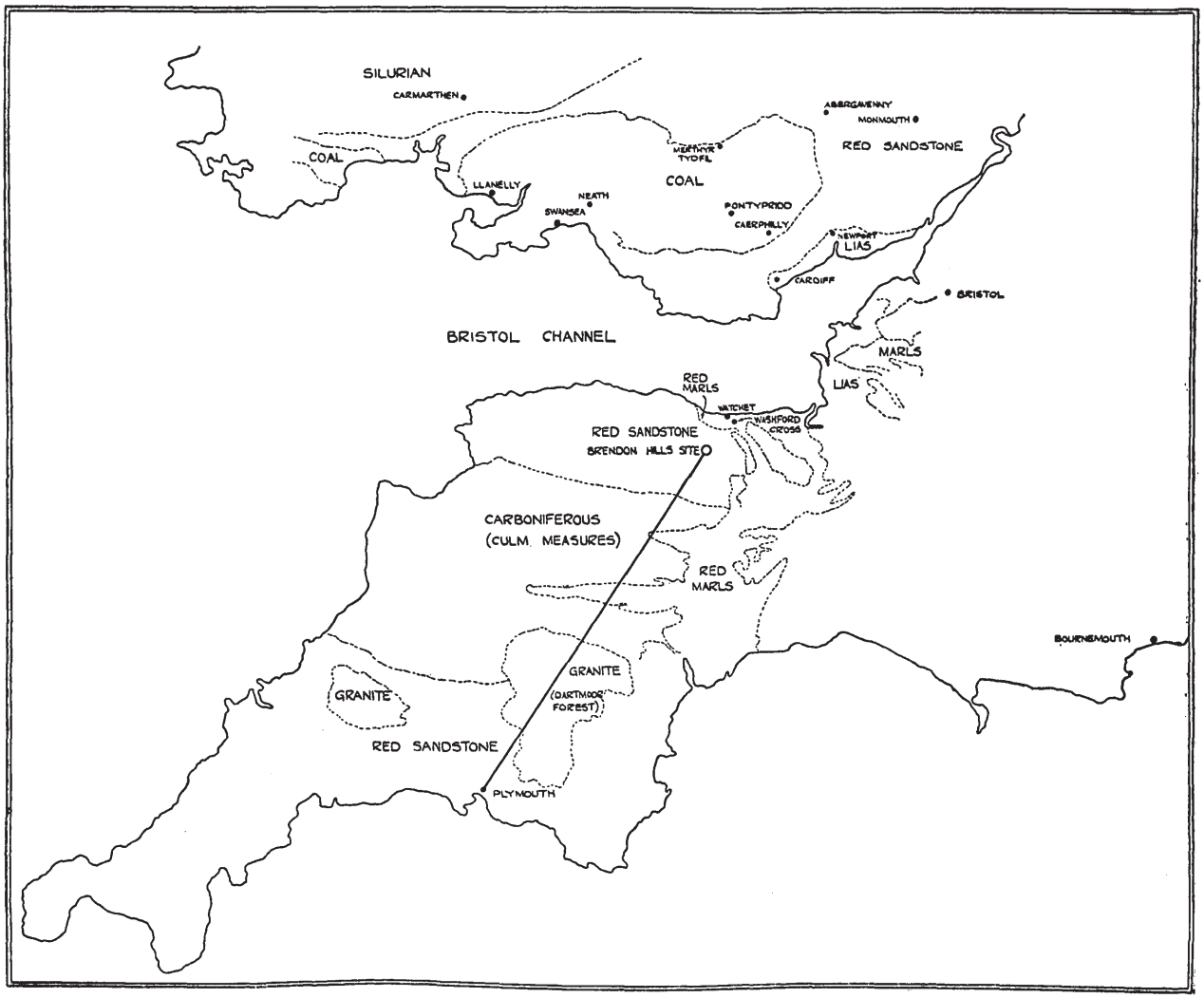

FIG. C.—Western Regional transmitter. Diagram showing geological strata encountered by direct line from Brendon Hills site to Plymouth.

found that after waves had passed over such a region there was always a tendency to fill up. This phenomenon has been observed by Van der Pol and others, and is to be expected on theoretical grounds.

Mr. A. J. Gill: Dealing with the second paper, what surprises me about these field-strength measurements in Cornwall is not that the results of the two observers are so different, but that they agree so well. J. H. Reyner's results apparently represent one or two readings taken at each spot, and I understand that the author also only took one reading at each point. Fig. 3 of the first paper shows that whereas on one day the field strength of a long-wave station is about 1 000 microvolts per metre at a certain point, a little later the intensity at the same point has risen to 3 000 microvolts per metre. This represents an increase of 300 per cent obtained by the same observer at the same spot, and we should therefore not be surprised at a difference of 300 per cent between the results of two independent observers obtained at different times and in different places on a much shorter wave. It seems doubtful whether the assumption is justified that there was no downcoming wave, as only two tests were made at the commencement of the work. When taking field-strength measurements on the road I have noticed that a movement of only 100 yards will cause a large difference—sometimes of the order of 50 per cent—in the reading obtained. Field-strength measurements taken as one moves away from a transmitting station do not give rise to a smooth curve. A series of peaks and valleys is obtained, and a curve has to be drawn through these in order to give some idea of the attenuation. I therefore think that the readings obtained by the author cannot be average values.

Lieut.-Col. H. P. T. Lefroy: As regards the signal strength from a given transmitting station observed at any given point, previous speakers have commented upon the effects of absorption in, and interference by reflection from, the Heaviside layer, and also of screening on, and dissipation in, the earth's surface. I suggest that it may prove necessary to take into consideration the effect of minerals underlying the point of observation, and perhaps even the whole path of the radiation measured. Certain phenomena seem to indicate that there is some hitherto unobserved radiation, or field of force, associated with all minerals and characteristic of each. In some cases this suggested effect may reduce (by some form of interference) the signal strength at points situated over mineral deposits, even when the latter are at a considerable depth. Chapman and Franklin* have recently developed a method of geophysical prospecting, based upon the observed variation of signal strength in the area under investigation, which may in time provide data in confirmation of the hypothesis suggested.

Mr. R. Naismith (*in reply*): The disagreement expressed by Dr. Smith-Rose with my opening sentence on page 121 of the first paper is not confirmed by his quotation. I have defined the short distances in that sentence as distances under 300 km. The extreme limits of distance in the curve quoted lie between 300 and 1 000 km; therefore the quotation is not applicable. I would, however, draw his attention to the words "in the summer"* which, though inadvertently omitted from the advance copies of the paper, are obviously implied throughout the paper. For instance, during winter even medium-distance transmission is subject to large daily variations and the comparison in the paper would then be impossible. I agree that it does not follow that because the energy has not come down at a short distance, it *will* come down a long way away, but my statement in the penultimate paragraph of page 124, col. 1, is that if a large amount of the energy from a transmitting station *is* returned close to that transmitting station, there will be less to reach the greater distances. These two statements clearly do not coincide. The values marked A and B in Figs. 3 and 4 are simply calculated values based on the formula $E_0 = 120\,\pi Ihf(d\lambda)$.

In discussing the second paper Dr. Smith-Rose finds it difficult to conceive that a hill 1 400 ft. high could cast a shadow persisting for 50 miles. The following extract from a paper by Ollendorff† on the subject shows that the shadow zone could still be observed at 4 times this distance. A model of a range of hills is represented by a semicircular cylinder, Fig. D. The waves are propagated perpendicularly to the direction of the range so that the range of hills always lies in a wave surface. If Z is a diffraction zone within which the diffracted wave shall be at least 10 per cent of the primary wave and ϵ is the dielectric constant of the obstacle, h the height of the obstacle and λ the wavelength considered, the following relation holds:—

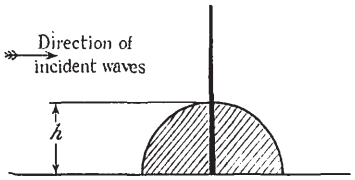

FIG. D.

$$\frac{Z}{\lambda} = (20\pi^2)^2\left(\frac{\epsilon - 1}{\epsilon + 1}\right)^2\left(\frac{h}{\lambda}\right)^4$$

Taking $\epsilon = 45$‡ and putting $h = 0{\cdot}27\,\lambda$ we get a diffraction zone of $220\,\lambda$ or 212 miles. Regarding the accuracy of measurement, I know of no standard of reference in this country, and owing to this regrettable circumstance refrain from further comment. I cannot agree that broken ground or variable subsoils could produce the symmetrical contours of Fig. 2.

In reply to Mr. Reyner, it is true that a complete exploration of the shadow should theoretically reveal certain closed contours, but as this effect has already been discussed at some length in the paper by Bown, Martin and Potter referred to in the text, I did not feel justified in undertaking the extra work which would be involved to complete the shadow picture.

I wish to thank Dr. Hollingworth for his valuable contribution to the subject of long-wave phenomena over short distances.

* "Applied Geophysics" (Board of Education), H.M. Stationery Office, 1931, p. 47.

* Since added for the *Journal*.

† *Elektrische Nachrichten-Technik*, 1931, vol. 8, p. 147.

‡ J. A. RATCLIFFE and F. W. G. WHITE: *Philosophical Magazine*, 1930, vol. 10, p. 667.

Captain Eckersley raises the question of the theoretical intensity. The curve shown in Fig. 3 is obtained from the Macdonald formula for the diffraction of electric waves round a perfectly reflecting obstacle. It is interesting to learn that his measurements at Sutton Bank, Yorkshire, provide such direct confirmation of my results in Devon. Judging by the position of many receiving stations, I should say this effect is not fully recognized.

The measurements quoted by Mr. Kirke were made on 300 metres and cannot therefore be directly compared. Dr. Smith-Rose and Mr. Barfield have shown* that so long as the subsoil is screened by a layer of vegetation it exerts no influence on the waves passing over it. In the discussion Dr. Smith-Rose rather suggests that this hypothesis may need some revision. The measurements in Cornwall were made on widely differing subsoils such as serpentine, magnetite, slate, sand, granite and tin. Therefore the entire absence of any of the sudden changes in the attenuation coefficient noticed by Mr. Kirke, and the smooth nature of the contours passing over this exceedingly varied geological structure, lend no support to the suggestion that the subsoil affects the contours of signal strength.

The variation of 300 per cent quoted by Mr. Gill was a very definite case of change of ionization in the reflecting layer. The assumption of no downcoming wave in Cornwall was not only dependent on the results of the two tests but was further substantiated by the total number of observations, since the resultant of the reflected wave and the wave diffracted over the surface of the earth would give rise to a stationary interference pattern on the ground.

The many causes of variation of signal strength have not yet been fully explored. Lieut.-Col. Lefroy mentions one of these which is at present being investigated by Chapman and Franklin with a view to its practical application. A complete statement of all the causes and the relative importance of each could not be made at present, but the effect described in the paper is so prominent that other effects which may be present are unnoticeable.

* *Proceedings of the Royal Society*, A, 1925, vol. 107, p. 587.